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THE ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE—II.

HABIT ILLUSTRATED BY MORPHOLOGY

WE have hitherto been considering the mnemic quality of movements; but, as I have attempted to show, morphological changes are reactions to stimulation of the same kind as these temporary changes. It is indeed from the morphological reactions of living things that the most striking cases of habit are, in my opinion, to be found.

The development of the individual from the germ-cell takes place by a series of stages of cell-division and growth, each stage apparently serving as a stimulus to the next, each unit following its predecessor like the movements linked together in an habitual action performed by an animal.

My view is that the rhythm of ontogeny is actually and literally a habit. It undoubtedly has the feature which I have described as preeminently characteristic of habit, viz., an automatic quality which is seen in the performance of a series of actions in the absence of the complete series of stimuli to which they (the stages of ontogeny) were originally due. This is the chief point on which I wish to insist—I mean that the resemblance between ontogeny and habit is not merely superficial, but deeply seated. It was with this conclusion in view that I dwelt, at the risk of being tedious, on the fact that memory has its place in the morphological as well as in the temporary reac-

tions of living things. It can not be denied that the ontogenetic rhythm has the two qualities observable in habit—namely, a certain degree of fixity or automaticity, and also a certain variability. A habit is not irrevocably fixed, but may be altered in various ways. Parts of it may be forgotten or new links may be added to it. In ontogeny the fixity is especially observable in the earlier, the variability in the later, stages. Mr. Darwin has pointed out that "on the view that species are only strongly marked and fixed varieties, we might expect often to find them still continuing to vary in those parts of their structure which have varied within a moderately recent period." These remarks are in explanation of the "notorious" fact that specific are more variable than generic characters—a fact for which it is "almost superfluous to adduce evidence."[1] This, again, is what we find in habit: take the case of a man who, from his youth up, has daily repeated a certain form of words. If in middle life an addition is made to the formula, he will find the recently acquired part more liable to vary than the rest.

Again, there is the wonderful fact that, as the ovum develops into the perfect organism, it passes through a series of changes which are believed to represent the successive forms through which its ancestors passed in the process of evolution. This is precisely paralleled by our own experience of memory, for it often happens that we can not reproduce the last learned verse of a poem without repeating the earlier part; each verse is suggested by the previous one and acts as a stimulus for the next. The blurred and imperfect character of the ontogenetic version of the phylogenetic series may at least remind us of the tendency to abbreviate by omission what we have learned by heart.

[1] "Origin of Species," 6th edition, p. 122.

In all bi-sexual organisms the ontogenetic rhythm of the offspring is a combination of the rhythms of its parents. This may or may not be visible in the offspring; thus in the crossing of two varieties the mongrel assumes the character of the prepotent parent. Or the offspring may show a blend of both parental characters. Semon[2] uses as a model the two versions of Goethe's poem—

Ueber allen Gipfeln, ist Ruh, in allen Wäldern, hörest du, keinen Hauch.
Ueber allen Gipfeln, ist Ruh, in allen Wipfeln, spürest du, kaum einen Hauch.

One of these terminations will generally be prepotent, probably the one that was heard first or heard most often. But the cause of such prepotency may be as obscure as the corresponding occurrence in the formation of mongrels. We can only say that in some persons the word "allen" releases the word "Wäldern," while in others it leads up to "Wipfeln." Again, a mixture of the terminations may occur leading to such a mongrel form as: "in allen Wäldern hörest du kaum einen Hauch." The same thing is true of music; a man with an imperfect memory easily interpolates in a melody a bar that belongs elsewhere. In the case of memory the introduction of a link from one mental rhythm into another can only occur when the two series are closely similar, and this may remind us of the difficulty of making a cross between distantly related forms.

Enough has been said to show that there is a resemblance between the two rhythms of development and of memory; and that there is at least a *prima facie* case for believing them to be essentially similar. It will be seen that my view is the same as that of Hering, which is generally described as the identification of memory

[2] "Die Mneme," 2d edition, pp. 147, 221, 303, 345.

and inheritance.[3] Hering says that "between the *me* of to-day and the *me* of yesterday lie night and sleep, abysses of unconsciousness; nor is there any bridge but memory with which to span them." And in the same way he claims that the abyss between two generations is bridged by the unconscious memory that resides in the germ cells. It is also the same as that of Semon and to a great extent as that of Rignano.[4] I, however, prefer at the moment to limit myself to asserting the identity of ontogeny and habit, or, more generally, to the assertion in Semon's phraseology, that ontogeny is a mnemic phenomenon.

Evolution, in its modern sense, depends on a change in the ontogenetic rhythm. This is obvious, since if this rhythm is absolutely fixed, a species can never give rise to varieties. This being so, we have to ask *in what ways* the ontogenetic rhythm can be altered. A habitual action, for instance, a trick learned by a dog, may be altered by adding new accomplishments; at first the animal will persist in finishing his performance at the old place, but at last the extended trick will be bonded into a rhythm of actions as fixed as was the original simpler performance. May we not believe that this is what has occurred in evolution?

We know from experiment that a plant may be altered in form by causes acting on it during the progress of development. Thus a beech tree may be made to develop different forms of leaves by exposing it to sunshine or to shade. The ontogeny is different in the two cases, and what is of special interest is that there exist shade-loving plants in which a structure similar to that of the shaded beech-leaf is apparently typical of the species, but on this point it is necessary to speak with caution. In the same way Goebel points out that in some orchids the assimilating roots take on a flattened form when exposed to sunlight, but in others this morphological change has become automatic, and occurs even in darkness.[5]

Such cases suggest at least the possibility of varieties arising as changes in or additions to the later stages of ontogeny. This is, briefly given, the epigenetic point of view.

But there is another way of looking at the matter—namely, that upheld by Galton and Weismann. According to this view ontogeny can only be changed by a fundamental upset of the whole system—namely, by an alteration occurring in its first stage, the germ cell, and this view is now very generally accepted.

The same type of change may conceivably occur in memory or habit, that is, the rhythm as a whole may be altered by some cause acting on the nerve-centers connected with the earlier links of the series. The analogy is not exact, but such an imaginary case is at least of a different type from a change in habit consisting in the addition of a new link or the alteration of one of the latest formed links. If we were as ignorant of the growth of human actions as we are of variation, we might have a school of naturalists asserting that all changes of habit originate in the earliest link of the series. But we know that this is not the case. On the other hand, I

[3] Everyone who deals with this subject must take his stand on the foundation laid by Hering in his celebrated address given at Vienna in 1870 and reprinted in No. 148 of Ostwald's "Exakt Klassiker." The passage quoted (p. 14) is from Samuel Butler's translation of Hering in "Unconscious Memory," 1880, p. 110. Butler had previously elaborated the view that "we are one person with our ancestors" in his entertaining book "Life and Habit," 1878, and this was written in ignorance of Hering's views.

[4] "Sur la transmissibilité des caractères acquis," Paris, 1906.

[5] Goebel's "Organography of Plants," part II., p. 285.

fully admit that the structure of an ovum may in this way be altered, and give rise to a variation which may be the starting-point of a new species.

But how can a new species originate according to an epigenetic theory? How can a change in the later stages of ontogeny produce a permanent alteration in the germ-cells? Our answer to this question will depend on our views of the structure of the germ-cells. According to the mnemic theory they have the quality which is found in the highest perfection in nerve-cells, but is at the same time a character of all living matter—namely, the power of retaining the residual effects of former stimuli and of giving forth or reproducing under certain conditions an echo of the original stimulus. In Semon's phraseology germ-cells must, like nerve-cells, contain engrams, and these engrams must be (like nerve-engrams) bonded together by association, so that they come into action one after another in a certain order automatically, *i. e.*, in the absence of the original stimuli.

This seems to me the strength of the mnemic theory—namely, that it accounts for the preformed character of germ-cells by the building up in them of an organized series of engrams. But if this view has its strength, it has also its weakness. Routine can only be built up by repetition, but each stage in ontogeny occurs only once in a lifetime. Therefore if ontogeny is a routine each generation must be chemically connected with the next. This can only be possible if the germ-cells are, as it were, in telegraphic communication with the whole body of the organism; so that as ontogeny is changed by the addition of new characters, new engrams are added to the germ-cell.

Thus in fact the mnemic theory of development depends on the possibility of what is known as somatic inheritance or the inheritance of acquired characters. This is obvious to all those familiar with the subject, but to others it may not be so clear. Somatic inheritance is popularly interesting in relation to the possible inherited effects of education, or of mutilations, or of the effects of use and disuse. It is forgotten that it may be, as I have tried to show, an integral part of all evolutionary development.

WEISMANN'S THEORY

Every one must allow that if Weismann's theory of inheritance is accepted we can not admit the possibility of somatic inheritance. This may be made clear to those unfamiliar with the subject by an illustration taken from the economy of an ant's nest or beehive. The queen,[6] on whom depends the future of the race, is cut off from all active experience of life: she is a mere reproducing machine, housed, fed and protected by the workers. But these, on whom falls the burden of the struggle for life and the experience of the world generally, are sterile, and take no direct share in the reproduction of the species. The queen represents Weismann's germ-plasm, the workers are the body or soma. Now imagine the colony exposed to some injurious change in environment; the salvation of the species will depend on whether or no an improved pattern of worker can be produced. This depends on the occurrence of appropriate variations, so that the queen bee and the drones, on whom this depends, are of central importance. On the other hand any change occurring in the workers, for instance, increased skill due to practise in doing their work or changes in their structure due to external conditions, can not possibly be inherited, since workers are absolutely cut off from the reproduction

[6] Nor do the drones share the activity of the workers.

of the race. According to Weismann, there is precisely the same bar to the inheritance of somatic change.

The racial or phyletic life of all organisms is conceived by him as a series of germ-cells whose activity is limited to varying, and whose survival in any generation depends on the production of a successful soma or body capable of housing, protecting, and feeding the germ-cell. Most people would *a priori* declare that a community where experience and action are separated must fail. But the bee's nest, which must be allowed to be something more than an illustration of Weismann's theory, proves the contrary.

It is clear that there must be war to the knife between the theory of Weismann and that of the somatists—to coin a name for those who believe in the inheritance of acquired characters. A few illustrations may be given of the strength of Weismann's position. Some trick or trivial habit appears in two successive generations, and the son is said to inherit it from his father. But this is not necessarily a case of somatic inheritance, since according to Weismann the germ-plasm of both father and son contained the potentiality of the habit in question. If we keep constantly in view Weismann's theory of continuity, the facts which are supposed to prove somatic inheritance cease to be decisive.

Weismann has also shown by means of his hypothesis of "simultaneous stimulation"[7] the unconvincingness of a certain type of experiment. Thus Fischer showed that when chrysalids of *Arctia caja* are subjected to low temperature a certain number of them produce dark-colored insects; and further that these moths mated together yield dark-colored offspring. This has been held to prove somatic inheritance, but Weismann points out that it is explicable by the low temperature having an identical effect on the color-determinants existing in the wing-rudiments of the pupa, and on the same determinants occurring in the germ-cells.

It does not seem to me worth while to go in detail into the evidence by which somatists strive to prove their point, because I do not know of any facts which are really decisive. That is to say, that though they are explicable as due to somatic inheritance, they never seem to me absolutely inexplicable on Weismann's hypothesis. But, as already pointed out, it is not necessary to look for special facts and experiments, since if the mnemic theory of ontogeny is accepted the development of every organism in the world depends on somatic inheritance.

I fully acknowledge the strength of Weismann's position; I acknowledge also most fully that it requires a stronger man than myself to meet that trained and well-tried fighter. Nevertheless, I shall venture on a few remarks. It must be remembered that, as Romanes[8] pointed out, Weismann has greatly strengthened his theory of heredity by giving up the absolute stability and perpetual continuity of germ-plasm. Germ-plasm is no longer that mysterious entity, immortal and self-contained, which used to suggest a physical soul. It is no longer the aristocrat it was when its only activity was dependent on its protozoan ancestors, when it reigned absolutely aloof from its contemporary subjects. The germ-plasm theory of to-day is liberalized, though it is not so democratic as its brother sovereign pangenesis, who reigns, or used to reign, by an elaborate system of proportional representation. But in spite of the skill and

[7] I borrow this convenient expression from Plate's excellent book, "Ueber die Bedeutung des Darwin'schen Selectionsprincips," 1903, p. 81.

[8] "An Examination of Weismann," 1893, pp. 169, 170.

energy devoted to its improvement by its distinguished author, Weismannism fails, in my opinion, to be a satisfactory theory of evolution.

All such theories must account for two things which are parts of a single process but may logically be considered separately: (1) The fact of ontogeny, namely, that the ovum has the capacity of developing into a certain more or less predetermined form; (2) the fact of heredity—the circumstance that this form is approximately the same as that of the parent.

The doctrine of pangenesis accounts for heredity, since the germ-cells are imagined as made up of gemmules representing all parts of the adult; but it does not account for ontogeny, because there seems to me no sufficient reason why the gemmules should become active in a predetermined order unless, indeed, we allow that they do so by habit, and then the doctrine of pangenesis becomes a variant of the mnemic theory.

The strength of Weismann's theory lies in its explanation of heredity. According to the doctrine of continuity, a fragment of the germ-plasm is, as it were, put on one side and saved up to make the germ-cell of the new generation, so that the germ-cells of two successive generations are made of the same material. This again depends on Weismann's belief that when the ovum divides, the two daughter cells are not identical; that in fact the fundamental difference between soma and germ-cells begins at this point. But this is precisely where many naturalists whose observations are worthy of all respect differ from him. Weismann's theory is therefore threatened at the very foundation.

Even if we allow Weismann's method of providing for the identity between the germ-cell of two successive generations, there remains, as above indicated, a greater problem—namely, that of ontogeny. We no longer look at the potentiality of a germ-cell as Caliban looked on Setebos, as something essentially incomprehensible, ruling the future in an unknown way—"just choosing so." If the modern germ-cell is to have a poetic analogue it must be compared to a Pandora's box of architectonic sprites which are let loose in definite order, each serving as a master builder for a prescribed stage of ontogeny. Weismann's view of the mechanism by which his determinants—the architectonic sprites—come into action in due order is, I assume, satisfactory to many, but I confess that I find it difficult to grasp. The orderly distribution of determinants depends primarily on their arrangement in the ids, where they are held together by "vital affinities." They are guided to the cells on which they are to act by differential divisions, in each of which the determinants are sorted into two unequal lots. They then become active, *i. e.*, break up into biophores, partly under the influence of liberating stimuli and partly by an automatic process. Finally the biophores communicate a "definite vital force" to the appropriate cells.[9] This *may* be a description of what happens; but inasmuch as it fails to connect the process of ontogeny with physiological processes of which we have definite knowledge, it does not to me seem a convincing explanation.

For myself I can only say that I am not satisfied with Weismann's theory of heredity or of ontogeny. As regards the first, I incline to deny the distinction between germ and soma, to insist on the plain facts that the soma is continuous with the germ-cell, and that the somatic cells may have the same reproductive qualities as the germ-cells (as is proved by the facts of regeneration); that, in fact, the germ-cell

[9] "The Evolution Theory," English translation, I., 373 *et seq.*

is merely a specialized somatic cell and has the essential qualities of the soma. With regard to ontogeny, I have already pointed out that Weismann does not seem to explain its automatic character.

THE MNEMIC THEORY

If the mnemic theory is compared with Weismann's views it is clear that it is strong precisely where these are weakest —namely, in giving a coherent theory of the rhythm of development. It also bears comparison with all theories in which the conception of determinants occurs. Why should we make elaborate theories of hypothetical determinants to account for the potentialities lying hidden in the germ-cell, and neglect the only determinants of whose existence we have positive knowledge (though we do not know their precise nature)? We know positively that by making a dog sit up and then giving him a biscuit we build up something in his brain in consequence of which a biscuit becomes the stimulus to the act of sitting. The mnemic theory assumes that the determinants of morphological change are of the same type as the structural alteration wrought in the dog's brain.

The mnemic theory—at any rate that form of it held by Semon and by myself—agrees with the current view, viz., that the nucleus is the center of development, or, in Semon's phraseology, that the nucleus contains the engrams in which lies the secret of the ontogenetic rhythm. But the mode of action of the mnemic nucleus is completely different from that of Weismann. He assumes that the nucleus is disintegrated in the course of development by the dropping from it of the determinants which regulate the manner of growth of successive groups of cells. But if the potentiality of the germ nucleus depends on the presence of engrams, if, in fact, its function is comparable to that of a nerve-center, its capacity is not diminished by action; it does not cast out engrams from its substance as Weismann's nucleus is assumed to drop armies of determinants. The engrams are but cut deeper into the records, and more closely bonded one with the next. The nucleus, considered as a machine, does not lose its component parts in the course of use. We shall see later on that the nuclei of the whole body may, on the mnemic theory, be believed to become alike. The fact that the mnemic theory allows the nucleus to retain its repeating or reproductive or mnemic quality supplies the element of continuity. The germ-cell divides and its daughter cells form the tissues of the embryo, and in this process the original nucleus has given rise to a group of nuclei; these, however, have not lost their engrams, but retain the potentiality of the parent nucleus. We need not, therefore, postulate the special form of continuity which is characteristic of Weismann's theory.

We may say, therefore, that the mnemic hypothesis harmonizes with the facts of heredity and ontogeny. But the real difficulties remain to be considered, and these, I confess, are of a terrifying magnitude.

The first difficulty is the question how the changes arising in the soma are, so to speak, telegraphed to the germ-cells. Hering allows that such communication must at first seem highly mysterious.[10] He then proceeds to show how by the essential unity and yet extreme ramification of the nervous system "all parts of the body are so connected that what happens in one echoes through the rest, so that from the disturbance occurring in any part some notification, faint though it may be, is conveyed to the most distant parts of the body."

A similar explanation is given by Nägeli.

[10] E. Hering in Ostwald's *Klassiker der exakten Wissenschaften*, No. 148, p. 14; see also S. Butler's translation in "Unconscious Memory," p. 119.

He supposes that adaptive, in contradistinction to organic, characters are produced by external causes; and since these characters are hereditary there must be communication between the seat of adaptation and the germ-cells. This telegraphic effect is supposed to be effected by the network of idioplasm which traverses the body, in the case of plants by the intercellular protoplasmic threads.

Semon faces the difficulty boldly. When a new character appears in the body of an organism, in response to changing environment, Semon assumes that a new engram is added to the nuclei in the part affected; and that, further, the disturbance tends to spread to all the nuclei of the body (including those of the germ-cells), and to produce in them the same change. In plants the flow must be conceived as traveling by intercellular plasmic threads, but in animals primarily by nerve-trunks. Thus the reproductive elements must be considered as having in some degree the character of nerve-cells. So that, for instance, if we are to believe that an individual habit may be inherited and appear as an instinct, the repetition of the habit will not merely mean changes in the central nervous system, but also corresponding changes in the germ-cells. These will be, according to Semon, excessively faint in comparison to the nerve-engrams, and can only be made efficient by prolonged action. Semon lays great stress on the slowness of the process of building up efficient engrams in the germ-cells.

Weismann[11] speaks of the impossibility of germinal engrams being formed in this way. He objects that nerve-currents can only differ from each other in intensity, and therefore there can be no communication of potentialities to the germ-cell. He holds it to be impossible that somatic changes should be telegraphed to the germ-cell and be reproduced ontogenetically—a process which he compares to a telegram despatched in German and arriving in Chinese. According to Semon[12] what radiates from the point of stimulation in the soma is the primary excitation set up in the somatic cells; if this is so, the radiating influence will produce the same effect on all the nuclei of the organism. My own point of view is the following. In a plant (as already pointed out) the ectoplasm may be compared to the sense-organ of the cell, and the primary excitation of the cell will be a change in the ectoplasm; but since cells are connected by ectoplasm threads the primary excitation will spread and produce in other cells a faint copy of the engram impressed on the somatic cells originally stimulated. But in all these assumptions we are met by the question to which Weismann has called attention—namely, whether nervous impulses can differ from one another in *quality*?[13] The general opinion of physiologists is undoubtedly to the opposite effect—namely, that all nervous impulses are identical in quality. But there are notable exceptions, for instance, Hering,[14] who strongly supports what may be called the qualitative theory. I am not competent to form an opinion on the subject, but I confess to being impressed by Hering's argument

[11] Weismann, "The Evolution Theory," 1904, Vol. II., p. 63; also his "Richard Semon's 'Mneme' und die Vererbung erworbener Eigenschaften," in the *Archiv für Rassen- und Gesellschafts-Biologie*, 1906. Semon has replied in the same journal for 1907.

[12] Semon, "Mneme," ed. I., p. 142, does not, however, consider it proved that the nucleus is necessarily the smallest element in which the whole inheritance resides. He refers especially to the regeneration of sections of *Stentor* which contain mere fragments of the nucleus.

[13] I use this word in the ordinary sense without reference to what is known as *modality*.

[14] "Zur Theorie der Nerventhätigkeit, Akademische Vortrag," 1898 (Veit, Leipzig).

that the nerve-cell and nerve-fiber, as parts of one individual (the neuron), must have a common irritability. On the other hand, there is striking evidence, in Langley's[15] experiments on the cross-grafting of efferent nerves, that here at least nerve impulses are interchangeable and therefore identical in quality. The state of knowledge as regards afferent nerves is, however, more favorable to my point of view. For the difficulties that meet the physiologist—especially as regards the nerves of smell and hearing—are so great that it has been found simpler to assume differences in impulse-quality, rather than attempt an explanation of the facts on the other hypothesis.[16]

On the whole it may be said that, although the trend of physiological opinion is against the general existence of qualitative differences in nerve-impulses, yet the question can not be said to be settled either one way or the other.

Another obvious difficulty is to imagine how within a single cell the engrams or potentialities of a number of actions can be locked up. We can only answer that the nucleus is admittedly very complex in structure. It may be added (but this not an answer) that in this respect it claims no more than its neighbors; it need not be more complex than Weismann's germ-plasm. One conceivable simplification seems to be in the direction of the pangenes of De Vries. He imagines that these heritage-units are relatively small in number, and that they produce complex results by combination, not by each being responsible for a minute fraction of the total result.[17] They may be compared to the letters of the alphabet which by combination make an infinity of words.[18] Nägeli[19] held a similar view. "To understand heredity," he wrote, "we do not need a special independent symbol for every difference conditioned by space, time or quality, but a substance which can represent every possible combination of differences by the fitting together of a limited number of elements, and which can be transformed by permutations into other combinations." He applied (*loc. cit.*, p. 59) the idea of a combination of symbols to the telegraphic quality of his idioplasm. He suggests that as the nerves convey the most varied perceptions of external objects to the central nervous system, and there create a coherent picture, so it is not impossible that the idioplasm may convey a combination of its local alterations to other parts of the organism.

Another theory of simplified telegraphy between soma and germ-cell is given by Rignano.[20] I regret that the space at my command does not permit me to give a full account of his interesting speculation on somatic inheritance. It resembles the theories of Hering, Butler and Semon in postulating a quality of living things, which is the basis both of memory and inheritance. But it differs from them in seeking for a physical explanation or model of what is common to the two. He compares the nucleus to an electric accumulator which in its discharge gives out the same sort of energy that it has received. How far this is an allowable parallel I am not prepared to say, and in what follows I have given Rignano's results in biological terms. What interests me is the conclusion that the impulse conveyed to the nucleus of the germ-cell is, as far as re-

[15] *Proc. Roy. Soc.*, 1904, p. 99. *Journal of Physiology*, XXIII., p. 240, and XXXI., p. 365.

[16] See Nagel, "Handbuch der Physiologie des Menschen," III. (1905), pp. 1–15.

[17] De Vries, "Intracellular Pangenesis," p. 7.

[18] I take this comparison from Lotsy's account of De Vries's theory. Lotsy, "Vorlesungen über Deszendenztheorien," 1906, I., p. 98.

[19] Nägeli's "Abstammungslehre," 1884, p. 73.

[20] For what is here given I am partly indebted to Signor Rignano's letters.

sults are concerned, the external stimulus. Thus, if a somatic cell (A) is induced by an external stimulus (S) acting on the nucleus to assume a new manner of development, a disturbance spreads through the organism, so that finally the nuclei of the germ-cells are altered in a similar manner. When the cellular descendants of the germ-cells reach the same stage of ontogeny as that in which the original stimulation occurred, a stimulus comes into action equivalent to S as regards the results it is capable of producing. So that the change originally wrought in cell A by the actual stimulus S is now reproduced by what may be called an inherited stimulus. But when A was originally affected other cells, B, C, D, may have reacted to S by various forms of growth. And therefore when during the development of the altered germ-cell something equivalent to S comes into play, there will be induced, not merely the original change in the development of A, but also the changes which were originally induced in the growth of B, C, D. Thus, according to Rignano, the germ-nucleus releases a number of developmental processes, each of which would, according to Weismann, require a separate determinant.

If the view here given is accepted, we must take a new view of Weismann's cases of *simultaneous stimulation, i. e.*, cases like Fischer's experiments on *Arctia caja*, which he does not allow to be somatic inheritance. If we are right in saying that, the original excitation of the soma is transferred to the germ-cell, and it does not matter whether the stimulus is transferred by "telegraphy," or whether a given cause, *e. g.*, a low temperature, acts simultaneously on soma and germ-cell. In both cases we have a given alteration produced in the nuclei of the soma and the germ-cell. Nägeli used the word *telegraphy* to mean a dynamic form of transference, but he did not exclude the possibility of the same effect being produced by the movement of chemical substances, and went so far as to suggest that the sieve tubes might convey such stimuli in plants. In any case this point of view[21] deserves careful consideration.

Still another code of communication seems to me to be at least conceivable. One of the most obvious characteristics of animal life is the guidance of the organism by certain groups of stimuli, producing either a movement of seeking (positive reaction[22]) or one of avoidance (negative reaction). Taking the latter as being the simplest, we find that in the lowest as in the highest organisms a given reaction follows each one of a number of diverse conditions which have nothing in common save that they are broadly harmful in character. We withdraw our hands from a heated body, a prick, a corrosive substance, or an electric shock. The interesting point is that it is left to the organism to discover by the method of trial and error the best means of dealing with a sub-injurious stimulus. May we not therefore say that the existence of pleasure and pain simplifies inheritance? It certainly renders unnecessary a great deal of detailed inheritance. The innumerable appropriate movements performed by animals are broadly the same as those of their parents, but they are not necessarily inherited in every detail; they are rather the unavoidable outcome of hereditary but unspecialized sensitiveness. It is as though heredity were arranged on a code-system instead of by separate signals for every movement of the organism.

It may be said that in individual life the penalty of failure is pain, but that the

[21] See Semon, *Archiv f. Rassen- und Gesellschafts-Biologie*, 1907, p. 39.

[22] See Jennings, "Behavior of the Lower Organisms."

penalty for failure in ontogenetic morphology is death. But it is only because pain is the shadow cast by Death as he approaches that it is of value to the organism. Death would be still the penalty of creatures that had not acquired this sensitiveness to the edge of danger. Is it not possible that the sensitiveness to external agencies by which structural ontogeny is undoubtedly guided may have a similar quality, and the morphological variations may also be reactions to the edge of danger. But this is a point of view I can not now enter upon.

It may be objected that the inheritance of anything so complex as an instinct is difficult to conceive on the mnemic theory. Yet it is impossible to avoid suspecting that at least some instincts originate in individual acquirements, since they are continuous with habits gained in the lifetime of the organism. Thus the tendency to peck at any small object is undoubtedly inherited; the power of distinguishing suitable from unsuitable objects is gained by experience. It may be said that the engrams concerned in the pecking instinct can not conceivably be transferred from the central nervous system to the nucleus of the germ-cells. To this I might answer that this is not more inconceivable than Weismann's assumption that the germ-cell chances to be so altered that the young chicken pecks instinctively. Let us consider another case of what appears to be an hereditary movement. Take, for instance, the case of a young dog, who in fighting bites his own lips. The pain thus produced will induce him to tuck up his lips out of harm's way. This protective movement will become firmly associated with, not only the act of fighting, but with the remembrance of it, and will show itself in the familiar snarl of the angry dog. This movement is now, I presume, hereditary in dogs, and is so strongly inherited by ourselves (from simian ancestors) that a lifting of the corner of the upper lip is a recognized signal of adverse feeling. Is it really conceivable that the original snarl is due to that unspecialized stimulus we call pain, whereas the inherited snarl is due to fortuitous upsets of the determinants in the germ-cell?

I am well aware that many other objections may be advanced against the views I advocate. To take a single instance, there are many cases where we should expect somatic inheritance, but where we look in vain for it. This difficulty, and others equally important, must for the present be passed over. Nor shall I say anything more as to the possible means of communication between the soma and the germ-cells. To me it seems conceivable that some such telegraphy is possible. But I shall hardly wonder if a majority of my hearers decide that the available evidence in its favor is both weak and fantastic. Nor can I wonder that, apart from the problem of mechanism, the existence of somatic inheritance is denied for want of evidence. But I must once more insist that, according to the mnemic hypothesis, somatic inheritance lies at the root of all evolution. Life is a gigantic experiment which the opposing schools interpret in opposite ways. I hope that in this dispute both sides will seek out and welcome decisive results. My own conviction in favor of somatic inheritance rests primarily on the automatic element in ontogeny. It seems to me certain that in development we have an actual instance of habit. If this is so, somatic inheritance must be a *vera causa*. Nor does it seem impossible that memory should rule the plasmic link which connects successive generations—the true miracle of the camel passing through the eye of a needle—since, as I have tried to show, the reactions of living things to their surroundings ex-

hibit in the plainest way the universal presence of a mnemic factor.

We may fix our eyes on phylogeny and regard the living world as a great chain of forms, each of which has learned something of which its predecessors were ignorant; or we may attend rather to ontogeny, where the lessons learned become in part automatic. But we must remember that the distinction between phylogeny and ontogeny is an artificial one, and that routine and acquisition are blended in life.[23]

The great engine of natural selection is taunted nowadays, as it was fifty years ago, with being merely a negative power. I venture to think that the mnemic hypothesis of evolution makes the positive value of natural selection more obvious. If evolution is a process of drilling organisms into habits, the elimination of those that can not learn is an integral part of the process, and is no less real because it is carried out by a self-acting system. It is surely a positive gain to the harmony of the universe that the discordant strings should break. But natural selection does more than this; and just as a trainer insists on his performing dogs accommodating themselves to conditions of increasing complexity, so does natural selection pass on its pupils from one set of conditions to other and more elaborate tests, insisting that they shall endlessly repeat what they have learned and forcing them to learn something new. Natural selection attains in a blind, mechanical way the ends gained by a human breeder; and by an extension of the same metaphor it may be said to have the power of a trainer—of an automatic master with endless patience and all time at his disposal.

FRANCIS DARWIN

[23] This subject is dealt with in a very interesting manner in Professor James Ward's forthcoming lectures on the "Realm of Ends." Also in his article on "Mechanism and Morals" in the *Hibbert Journal*, October, 1905, p. 92; and in his article on Psychology in the "Encyclopedia Britannica," 1886, Vol. XX., p. 44.

THE ANALYST, THE CHEMIST AND THE CHEMICAL ENGINEER[1]

LET us consider that the terms, the analyst, the chemist and the chemical engineer, represent those members of the chemical profession who devote their time to the practical and industrial aspects of the science, as contrasted with the teachers of chemistry and the workers in abstract research.

The teacher of chemistry and the man of abstract research may be compared to the exciter, the industrial chemist to the dynamo, which supplies whatever power is to be derived from the science of chemistry, to the industrial world.

It is essential that the industrial chemist and the teacher should work closely together, that each should know the aims and needs of the other, if the power of chemical science is to be developed to its full capacity.

There is no more important member of the community to-day than the chemist. I doubt that there ever were more important members of the community even in the more primitive conditions of society than the men who smelted the iron, and tanned the leather, or the women who wrought and burned the earthen pots and dyed the fibers for weaving. And these technologists were the early representatives of the chemical profession, they were the industrial chemists of those early times—chemists to this extent: they knew the properties of certain substances and the chemical transformations in certain directions which these substances were capable of undergoing.

The soldier, the priest and the medicine

[1] Address delivered before the New Haven meeting of the American Chemical Society.

man were the members of the primitive horde to whom gravitated the honors, the wealth and the positions. But those who worked up natural resources, those who started manufacture, who developed industry and created wealth, were these not in reality the important members of the early community? Were they not in fact the founders of our modern civilization?

The reader of histories of chemistry is impressed at times by the emphasis laid upon the early speculators in chemistry who were all too apt in forming hypotheses. The known facts of chemistry, as shown by their applications in the industries, for some periods, have not been so well investigated and presented—I do not wish to disparage the hypothesis, but it seems as though the history of industrial chemistry had not had its due. When the shadow of the middle ages was lifting, in the fourteenth, fifteenth and sixteenth centuries, and inquisitive physicians and priests and others began to investigate and write on chemical subjects they found in the chemical industries of that time a wealth of material. Through the times of church and military domination, through the period of alchemical investigation and alchemical fraud, the industries appear to have maintained their integrity and on the whole to have handed down traditionally the sure knowledge of those who had gone before, along with additions as additional knowledge was acquired.

There is still a great opportunity for the man who will trace the development of industrial chemistry through all time and show its continuous and logical development. That there was such a continuous development in spite of migration, war, pestilence and theocracy, one must feel certain. The industrial man usually escapes many of the vicissitudes of life except poverty and work. It is easy for priest and soldier to quarrel with the man of strongly expressed ideas and of great self assertion, with the man striving for wealth and power, difficult with the simple manufacturer of raw materials into various commodities. It would not do to say that the manufacturer fared well through all the changing fortunes of ancient and medieval history, but we can well believe that in spite of adverse conditions he maintained his processes, added to them, and transmitted them to his successors by the traditional route.

The chemical technologist, represented by the early workers in chemical industries, preceded the industrial chemist as we know him to-day. Unarmed by systematic knowledge, unversed in the definite methods used to-day in investigating chemical industrial problems, he yet developed chemical industry in some instances to a condition which has not been modified in essential particulars, by the accumulated scientific knowledge of the present time. Consider, for example, the soap industry. The world over, soap is boiled to-day essentially as it was in the sixteen hundreds, before the birth of modern chemistry, two hundred years before the composition of fats was known, two hundred years before the nature of the alkalies or the process of saponification were understood. To-day we recover glycerol and salt, we use more soda and less potash, we are more skilled in the use of fillers in the manufacture, but on the whole the procedure is the same empirical one which has been used for three hundred years. In recent months something has been accomplished by Leimsdörfer in Germany and Mercklen in France, to rescue soap manufacture from empiricism, but the day is yet distant when scientific practise will be substituted for practical experience.

In many chemical industries the same

conditions prevail or prevail to a large extent. Think for a moment of the iron industry and other metallurgical branches, of the glass and particularly the pottery industry, and of other lines, and it is not difficult to see that the chemist has already much work laid out for him. The traditional empirical knowledge of chemical manufacture has always proved a rich field for the chemical investigator and it is as broad and rich and fruitful to-day as ever.

It is the chemist's prominent connection with the industrial life which characterizes our civilization, which gives him the preeminence which he enjoys in our own times. Along with the engineer he is the creator for good or bad of whatever originality there is in our modern life. Contrast his position in the community with that of other scientific men, the zoologist, the geologist, the botanist, and you are impressed with his somewhat closer connection with modern affairs and tendencies than theirs.

As I have stated there is no more important member of the community to-day than the chemist, and I think there is none who feels his importance less. Up to a certain point modesty is a pleasing attribute and desirable, but modesty which through inaction fails to obtain its just reward in position and emolument is scarcely so commendable.

We have heard the broader education of the chemist and the chemical engineer treated of with the fulness and the insight which the subject deserves, by members of the profession who spoke with authority. I want to speak for the broader *life* of the chemist. The broader life in the sense of his coming more in contact with men and affairs and tendencies of the times, of coming to play the important part he should play in the modern world. If I can indicate some of the points of contact, some of the opportunities in America, I shall be satisfied. And first I desire to consider the work of the analyst. The analyst is a chemist who, by various devices called methods of analysis, endeavors to ascertain the composition of substances. The chemical work of all manufacturing plants is mainly analytical and the analyst has come to be a great and important factor in the industrial world.

When one analyst meets another, he usually asks him the question "What method do you use?" and the reply is "I have a method of my own."

I trust that in my remarks I shall not in any way discourage originality among analysts, but I want to direct your attention to some of the consequences of individualism in matters of chemical analysis and suggest a remedy for them.

It will occcur to anybody at once that if a person has a chemical method which is worth applying, he ought convince others of its excellence. Much adverse criticism has been aimed at chemists and chemistry through their failure to deliver agreeing analytical results. A part of the trouble is due to incompetent analysts and a part to unsatisfactory methods and methods which are not uniform. The incompetent man is apparently a necessary evil in every line of work and is difficult to eliminate. Possibly an institute of chemists with strict qualifications would help in this matter. But there is no good reason why we should not have well-tried and uniform methods of analysis.

The reactions on which analytical methods are based are for the most part old and well known. The working out of a method is usually done by a chemist of inventive ability, who is able by various means to make a reaction complete and definite enough so that it will yield quantitative results. We may grant that such work

can not be done by every analyst, but we must admit that every analyst is capable of using an analytical method and also of proving its value or the reverse: otherwise he is not worthy the name of analyst.

The great majority of analytical chemists are not inventors of analytical methods, they are only users of them. Usually when an analyst says: "I use a method of my own," he means that he has perhaps substituted a porcelain for a platinum crucible or one form of burette for another or altered the time of precipitation—in other words, introduced an unimportant variation into a standard method and named it his own. And thus we have in laboratories working in the same line a host of modifications of standard methods, which, while they do not necessarily cause a great difference in analytical results, do introduce a dangerous principle. And in some cases the application of this principle, that every analyst is privileged to modify methods as he chooses, leads to absolutely incorrect methods, as numerous cases which might be cited prove.

The remedy for this state of affairs is the recognition of the principle that no chemist is privileged to use any method or modification of a method which has not the approval of a representative committee of his brother chemists, authoritatively appointed by a chemical society to investigate the method. Further, that when a method is adopted by such a committee, no deviation from it should be allowable in the practise of any individual. In short, we should have standard methods of analysis and adhere to them.

The argument against these ideas will be that we do not want cook-book recipes in place of general analytical methods. If analytical chemistry is to be developed in a scientific way it must be made to yield absolute and not comparative results. There is much justice in these views. They were the views I held for a number of years. But it should be understood that these principles in their application and these general methods in hands less expert than they should be, have brought down upon the heads of chemists much indiscriminate criticism. A merely practical or business man does not have and can not be expected to have any particular insight into chemical methods, nor can he be expected to be able to judge of chemists. So long as he deals with one chemist only, and if this one happens to be a good analyst and to have good judgment, his faith in the profession may remain unchallenged. But if, for the sake of checking results he sends out ten identical samples to ten different chemists and receives ten reports of varying degrees of disagreement, his faith is likely to receive a shock. If he repeats the experiment and fares no better and if he finds that succeeding repetitions do not bring reasonable agreement, he may come to have in time nothing but cynical remarks to make about chemists and the science of chemistry. Of course the inaccurate and inexperienced analyst is a factor in the problem and must be considered, and while other means must be devised to eliminate this factor, as a practical necessity the large chemical societies must take up consistently and determinedly the problem of the unification of methods of analysis. The time has come when no analytical method can be left to individual judgment. Individual differences and individual preferences must be abandoned in favor of the greater good which will come from concerted action and unification in methods of analysis.

Something has already been done in the line I have suggested. The Agricultural Chemists, the Mechanical and Civil Engineers, the Leather Chemists' Association,

the Society for Testing Materials, the National Fertilizer Association and individual firms employing many chemists or operating several laboratories, have done or are doing work along the line of unifying methods. Those whom I have mentioned are not the only ones who are doing this important work, but if they alone were engaged upon it it could fairly be assumed that perfect unification might not result. To be sure, they might not all be working on the same things, but it is certain that much more could be accomplished if the work were being done by one central organization such as our Chemical Society.

There are great advantages in work on unification of methods. It trains the chemist in the art peculiar to chemistry. The work does not require men of great or special talents; on the other hand, it can be done satisfactorily by good careful analysts of ordinary skill and common sense. What it does require before everything else is organization and after this reasonably careful analysts and organized effort. There is no body better able to take up the work in so far as it concerns industrial methods than the Division of Industrial Chemists and Chemical Engineers.

I should say that work of this kind should be considered under eight heads: (1) definitions of all terms requiring definition which come up during the progress of the work; (2) methods of sampling, which, if correct results are to be delivered, are fully as important as correct analyses; (3) uniformity in reporting analyses; (4) methods of analysis themselves—that is methods recommended; (5) other methods which deserve mention but which are not recommended; (6) comments on the methods recommended, possibly detailing the results of a committee's analytical work; (7) publication in convenient and suitable form so that the results may reach all who are interested; (8) provision for a permanent committee to keep the work alive and up to date.

If some such plan as this is carried out, every chemist in the country who is called upon to do analytical work in a given line, will know where to go for approved methods of analysis, and while this will not assure the public, in the absence of capable chemists, of accurate results, it will at least solve a part of the problem. In regard to a distinguishing mark which would guide the public in the selection of competent analysts, possibly a properly organized institute of chemistry would be able to set such a stamp upon a man. But no institute can be considered as worthy of its high calling unless it is organized from among the acknowledged representative leaders of the profession. Mediocre men at the head of an institute of chemistry can do little for the movement which we all must hope will in due season come to pass.

In regard to publication, I suppose there will be some who will say when the work is in full blast—if it ever is—"Nothing in the Journal but methods." But in the first place not all the detailed work need be printed, and if it appears necessary to print a good deal, I can only say that it is important work—as important for the general good of the chemical profession as any research now being conducted. Further, it is not only desirable work, but, as things stand now, it is necessary work, which we can not evade if we would. We as analysts will have to admit that through lack of enterprise or for some other reason we have in some cases, and I am afraid the cases are numerous, allowed the matter of commercial analysis to be forced upon our attention by manufacturers and business men, instead of foreseeing and meeting these demands. Content with discoveries in pure science and in the life within the labora-

tory, we have at times held too much aloof from the needs of the manufacturing and commercial community in which we dwell. The analyst has done much, but we may easily believe that he can do more.

By the unmodified term *chemist* in the industrial sense, we may understand one who does more than analytical work, but who has relatively little to do with construction or industrial operations on the large scale. He may be a consulting man, a research man and an analyst besides these, or in charge of a laboratory employing a number of men. Whatever his particular line of work, there are a number of his class who appear to come in contact too seldom with chemists in other lines, with men of affairs, and with the activities of their community. Their time is spent in their laboratory or in their dwelling. Their lives are, in the familiar phrase, too narrow. There is such a thing as development by indirection, and who shall say that that man is not literally a better chemist who is more active in entirely different lines during a portion of his day? I say this particularly to the younger men who are industriously working their way up in large laboratories—get in touch with business men and methods and with merely practical manufacturers. Such associations lead to new points of view and are most beneficial and suggestive.

The sadly abused term "chemical engineer" may even yet be rescued from disaster and placed where it belongs, describing that adequately trained chemist who is capable of applying chemistry where construction work and operation are required. The chemist who is an engineer has much to answer for, and when I use the term I mean the one who is at least as much a chemist as he is an engineer, and not merely an engineer who, by contact with chemists or laboratories, has picked up a vague idea of chemical methods and problems. Engineering is extremely attractive to the younger chemist on account of its spectacular works and there is a little danger of his over-estimating it as a profession and under-estimating his own. This attitude will easily be outgrown with age, but that it is a factor in diverting men from the serious study of chemistry after leaving the university is unquestionable. Great are the works of the chemical engineer, but even greater the opportunities. I shall try to indicate what I consider some of them.

The chemical engineers have let go and are still letting go many opportunities. They have allowed the civil and the mechanical engineers to appropriate fields peculiarly their own. For example, water and sewage purification, fuels and smoke consumption. They have allowed the engineer, by his greater enterprise, to enter and appropriate to a large extent many kinds of chemical manufacture on the large scale. By chemical manufacture I do not mean the manufacture of chemicals such as acids, alkalies and salts alone, but any manufacture which is based upon chemical change. Many of the very old industries such as ceramics and metallurgy are pre-eminently chemical industries, but it would seem in many cases as though they were conducted by engineers with the chemist hired as an aid in a minor capacity. And when I make this statement please understand it is not a criticism of the engineer but of the chemist.

There may be some who will say, as I have heard it said, that the problems connected with the lines of work I have mentioned are more of a mechanical than a chemical kind, or at least the chemical problems connected therewith are less difficult of solution than the mechanical. It seems equally foolish to make a claim of

this sort and to answer it, but since the point has come up, a few suggestions may not be out of place. In the purification of sewage, it is true that there are needed well-designed conduits, tanks, filters, holding basins, etc., but it is equally true that the problem is from beginning to end a chemical one, whether precipitation methods or bacterial methods are used. You may say that bacteriology belongs to the biologist, but I think it is true that the problems connected with technical mycology are so largely chemical in nature that the chemist has at least an equal claim to them with the biologist. In bacterial sewage purification, we are not dealing with pure cultures; we supply the proper chemical conditions of oxidation or reduction, of alkalinity, etc., and assume that if the conditions are right the expected reactions under the influence of microorganisms will take place. If any engineer who is not a thorough chemist has a proper conception of the chemistry of sewage purification, I have not heard of him or read his works. I need not say more except that sewage works are usually constructed under the superintendence of engineers who hire analysts to make chemical determinations for them.

The problems connected with fuels and smoke consumption are chemical throughout, and again it is the exceptional engineer who has an adequate understanding of them; yet it can not be denied that the field belongs to the engineer at the present time by right of possession. The problem of smoke consumption was first adequately treated by an engineer and while we say now, glibly enough, that the solution of the problem lies in bringing the gases and solids in the furnace in contact with a sufficient air supply at a sufficiently high temperature, the problem was not so simply stated a few years ago. The problem is solved now at the cost of fire brick frequently renewed, but I am afraid the chemists' contribution to its solution was smaller than it should have been.

In conclusion, I trust that the future will see a closer contact between the votaries of the pure science of chemistry, the teachers of chemistry, the industrial chemists and the community at large. In that union lies the future successful development of the science and profession of chemistry.

W. D. Richardson

PRESENTATION TO PROFESSOR GOLDSCHMIDT

Professor Victor Goldschmidt, of the University of Heidelberg, to-day the foremost crystallographer, was, on his fifty-fifth birthday, presented with a silver punch-bowl by his former students in the United States and Canada. It is doubtful if any teacher of mineralogy either in America or Germany has instructed so many Americans who have since occupied positions of prominence having relation to the geological sciences. The following persons, twenty-five in all, contributed to the gift and signed the letter of birthday felicitation: M. B. Baker, Queens University (Kingston); Dr. Florence Bascom, professor of geology, Bryn Mawr College; Reginald W. Brock, acting director, Geological Survey of Canada; Dr. Hermon C. Cooper, associate professor of chemistry, Syracuse University; Dr. Reginald A. Daly, professor of geology, Massachusetts Institute of Technology; C. W. Dickson, Queen's University; Dr. William E. Ford, Jr., assistant professor of mineralogy, Sheffield Scientific School; Dr. C. H. Gordon, professor of geology, University of Tennessee; Dr. W. F. Hillebrand, U. S. Geological Survey; Dr. Wm. H. Hobbs, professor of geology, University of Michigan; Dr. T. A. Jaggar, Jr., professor of geology, Massachusetts Institute of Technology; Dr. A. C. Lawson, professor of geology and mineralogy, University of California; Dr. E. B. Mathews, professor of mineralogy, Johns Hopkins University; Dr. W. C. Mendenhall, U. S. Geological Survey;

Dr. W. G. Miller, provincial geologist, Ontario; William Nicol, professor of mineralogy, Queen's University; Dr. Chas. Palache, assistant professor of mineralogy, Harvard University; Dr. Joseph W. Richards, professor of metallurgy and mineralogy, Lehigh University; Walter S. Landis, Lehigh University; Dr. H. Monmouth Smith, professor of chemistry, Syracuse University; J. S. Stanley-Brown, editor Geological Society of America; Dr. Frank R. Van Horn, professor of mineralogy and geology, Case School of Applied Science; Dr. T. L. Walker, professor of mineralogy and petrography, University of Toronto; Dr. Fred E. Wright, Carnegie Institution; Dr. C. W. Wright, U. S. Geological Survey.

SCIENTIFIC NOTES AND NEWS

AT the Put-in-Bay meeting of the Astronomical and Astrophysical Society of America, the following officers were elected for the ensuing year:

President—E. C. Pickering.
First Vice-president—G. C. Comstock.
Second Vice-president—W. W. Campbell.
Secretary—W. J. Hussey.
Treasurer—C. L. Doolittle.
Councilors—Ormond Stone, W. S. Eichelberger, Frank Schlesinger, W. J. Humphreys.

A committee was appointed with power to determine the time and place of the next meeting.

THE British Ornithologists' Union will celebrate its fiftieth anniversary in December next, when gold medals will be presented to the four surviving original members: Dr. F. Du Cane Godman, F.R.S., Mr. P. S. Godman, Mr. W. H. Hudleston, F.R.S., and Dr. P. L. Sclater, F.R.S.

DR. JOSIAH ROYCE, of Harvard University, gave one of the principal addresses before the third International Philosophical Congress, which began its sessions at Heidelberg on September 1.

THE First International Moral Educational Congress is being held at the University of London, September 25–29, under the presidency of Professor Michael E. Sadler.

PROFESSOR D. J. HAMILTON, F.R.S., has, in consequence of ill health, resigned the chair of pathology in the University of Aberdeen.

MR. F. B. SMITH, director of agriculture for the Transvaal, is visiting England.

DR. EMIL KRAEPELIN, professor of psychiatry at Munich, who has been visiting this country, has returned to Germany.

DR. J. C. BOSE, professor in the University of Calcutta, India, author of "Response in the Living and Non-living," "Plant Response, as a Means of Physiological Investigation" and of "Comparative Electro-physiology," has been lecturing for the past few months on the continent and in England on the phenomena as brought out by his methods of experimentation. He expects to visit this country during October and November, and wishes to visit the more prominent institutions of the east and middle west. He will be very glad to lecture on his researches free of charge to university audiences or before scientific societies. Any institution that may wish to make arrangements for a series of three or four lectures by Dr. Bose, may address him in care of Mr. R. N. Tagore, Box 135, University Station, Urbana, Ill.

PROFESSOR C. H. HITCHCOCK will leave for Hawaii on the first of October. He goes to complete his book upon the Hawaiian volcanoes, which is to be published by the Hawaiian Gazette Company of Honolulu. Kilauea was never in better condition for study than now. The great pit is gradually filling up, and when the hydrostatic pressure of the column is too great to be maintained in its place the lava will escape into some unseen subterranean caverns, if it does not flow out at the surface on the lower ground.

BRIGADIER GENERAL JAMES ALLEN, chief of the United States Signal Corps, attended the International Electrical Congress at Marseilles, France, from September 14 to 19, as a representative of the United States army. Incidentally he will make a general investigation of what is being done in the development of war balloons and aeroplanes.

PRESIDENT CHARLES W. ELIOT, of Harvard University, is to deliver, on October 15, the ad-

dress to mark the opening of the Brooklyn Institute of Arts and Sciences, on which occasion his subject will be "The Building and Administration of a Modern City."

The president of the British Local Government Board has arranged for the making of the following additional researches in connection with the annual grant voted by parliament in aid of scientific investigations concerning the causes and processes of disease: 1. A chemical and bacteriological investigation by Mr. C. G. Moor, M.A., F.I.C., and Dr. Hewlett, professor of pathology at King's College, London, as to the influence of softening and of other chemical processes on the purity of water supplies from the chalk, as shown in actual experience and under experimental conditions. 2. An investigation by Professor Sidney Martin, F.R.S., into the powers of production of disease possessed by certain streptococci and by the poisonous substances produced by them, in continuance of previous investigations by him on the same subject.

We much regret to record here the death of Lieutenant Thomas E. Selfridge and the injury to Orville Wright in the aeroplane experiments, which up to that time had proceeded so auspiciously.

Dr. Charles Harrington, professor of hygiene in the Harvard Medical School and chairman of the Massachusetts State Board of Health, died suddenly in England on September 11, at the age of fifty-two years.

The death is announced of the Earl of Rosse, F.R.S., who, like his father, made valuable contributions to astronomy.

M. E. Mascart, since 1871 director of the French Meteorological Office, has died at the age of seventy-one years.

Dr. Ernst Ebermeyer, formerly professor of agriculture at Munich, has died at the age of seventy-nine years.

Dr. Hermann von Peetz, docent for geology at St. Petersburg, died on July 18, as the result of an accident while engaged in geological explorations.

We regret also to record the death of M. Auguste Daguillon, professor of botany at Paris, and of Prince Iwan Romanowitsch Tarchanow, professor of physiology at the Military Medical College at St. Petersburg.

The growth of the American Chemical Society has been so rapid this year that the publications for the early part of the year are exhausted. As new members are still coming in rapidly and are entitled to back *Journals* for this year on payment of their dues the Council has by the force of circumstances been obliged to grant half-year membership with half year dues for the last half of 1908 only. The size of the editions of the *Journal* and the *Abstracts* has been largely increased and the difficulty will probably not arise again.

It is announced that plans have been filed for the main hospital building and isolation annex of the Rockefeller Institute for Medical Research. The main building is to be a seven-story brick edifice, and the isolation wards will be in a two-story building connected with the main building by steel bridges. The estimated cost of the hospital is $350,000, the isolation annex $50,000, and the additional power house $4,000.

It is stated in *Nature* that the herbarium formed by Mr. Duthie, and hitherto quartered at Saharanpur, has been transferred to the Imperial Forest Institute, Dehra Dun; any correspondence in connection with it should be addressed to the imperial forest botanist of that institute.

The Second Annual Congress of the Playground Association of America met at the American Museum of Natural History, New York, from September 8 to 12. The association endeavors to show that properly supervised playgrounds help to produce good citizens, and an effort will be made to interest every American city in playgrounds.

In view of the very rapid development and progress of aerial navigation, it is proposed to establish a section at the Royal Polytechnic School of Naples, in which young engineers shall be trained in all that refers to the problems of flight, so far as it is known, both from a theoretical and practical point of view.

THE condition on September 1, with comparisons, of the various crops investigated by the Bureau of Statistics of the United States Department of Agriculture is as follows:

Crops	September 1			Aug. 1, 1908
	1908	1907	10 year Aver.	
Corn	79.4	80.2	81.0	82.5
Spring wheat	77.6	77.1	77.9	80.7
Oats	69.7	65.5	30.7	76.8
Barley	81.2	78.5	83.5	83.1
Buckwheat	87.8	77.4	86.5	89.4
Tobacco	84.3	82.5	83.7	85.8
Flaxseed	82.5	85.4	87.0	86.1
Rice	93.5	87.0	88.8	94.6
White potatoes	73.7	80.2	80.8	82.9
Sweet potatoes	88.7	85.7	85.3	88.8
Tomatoes	82.5	82.9	....	84.5
Cabbage	80.3	85.2	....	84.5
Onions	85.8	88.0	....	88.4
Beans	82.8	82.7	....	....
Apples	52.3	34.7	54.7	52.2
Peaches	67.5	30.7	53.0	67.1
Grapes	84.6	81.1	83.0	87.1
Pears	74.1	....	....	70.6
Watermelons	80.8	76.3	....	79.5
Cranberries	67.7	77.7	....	....
Oranges	88.2	84.1	....	89.1
Lemons	92.9	91.4	....	93.0
Sugar cane	91.3	94.1	90.4	88.3
Sorghum	85.5	82.4	....	85.4
Sugar beets	86.0	92.4	....	87.3
Broom corn	76.6	82.8	....	80.3
Hemp	73.0	85.9	....	77.4
Hops	79.3	88.5	....	86.4
Peanuts	86.0	85.6	....	85.5
Cotton (August 25)	76.1	72.7	73.9	83.0
Alfalfa (production)	90.7	91.8	....	88.8
Cloverseed (Acreage compared with preceding year, per cent.)	120.0	99.6	....	....
Cloverseed (condition)	89.7	76.5	.. ..	89.7
Millet	86.9	84.4	....	86.3
Kafir corn	85.1	83.1	....	....
Hay, yield per acre, tons	1.52	1.45	1.44	....
Hay, yield, tons (000 omitted)	67,743	63,677	....	....
Hay, quality	94.5	90.4	....	....
Rye, yield per acre, bu.	16.4	16.4	15.8	....
Rye, yield, bu. (000 omitted)	30,921	31,566	....	....
Rye, quality	92.7	91.6	....	....
Stock hogs, No. compared with preceding year	92.5	10.06	....	....
Stock hogs, condition	94.5	19	....	....

THE following facts were taken by Vice-consul James L. A. Burrell, of Magdeburg, from a brochure by Dr. Ernst Friedrich, of the German commercial high school at Leipzig: The world's lumber trade amounts to $285,600,000 annually, of which the United States furnishes about 20 per cent, Austria-Hungary 19 per cent., Russia 16 per cent., Canada 13 per cent., Sweden 18 per cent., Finland 10 per cent., Norway 4 per cent. and Roumania also a small quantity. The countries importing wood are those on the highest economical plane, which were themselves in earlier times densely wooded, but whose forests have been denuded to a greater or less extent to make room for agriculture, industry, etc. Only 4 per cent. of the territory of Great Britain is covered with forests, and during the year 1906 that country imported lumber to the value of $135,561,750. Germany has still 26 per cent. of its territory covered by forests, but imported in 1906 lumber valued at $61,285,000. Belgium and the Netherlands, that have but 8 per cent. forest lands, Denmark, that has 7 per cent., France and Switzerland, with a small percentage of forest land, are compelled to import lumber. Besides these countries, those lands lying on the dry western side of the subtropical zone lacking forests are forced to import wood. Egypt imports wood and coal to the value of about $16,660,000 annually; Algeria, Tunis, Spain, Portugal (3 per cent. forest land), Italy, Greece (with 9 per cent. forest land), the eastern part of Asia, British South Africa, the western parts of Chile and Peru, the Argentine Republic and Australia, all poor in wood, are dependent upon import.

THE Royal Commission on Sewage Disposal, appointed by the British government in 1898, has issued its fifth report, which deals chiefly with the relative merits of the various methods which are available for the purification of sewage of towns. The commissioners have held 144 meetings and called before them a large number of witnesses. A number of local authorities have carried out experimental investigations in association with the commission, the members of which have personally inspected a large number of sewage

works. The general conclusion of the commissioners is as follows: "We are satisfied that it is practicable to purify the sewage of towns to any degree required, either by land treatment or by artificial filters, and that there is no essential difference between the two processes, for in each case the purification, so far as it is not mechanical, is chiefly effected by means of microorganisms. The two main questions, therefore, to be considered in the case of a town proposing to adopt a system of sewage purifications are: First, what degree of purification is required in the circumstances of that town and of the river or stream into which its liquid refuse is to be discharged? Second, how the degree of purification required can, in the particular case, be most economically obtained? . . . We may state that we know of no case where the admixture of trade refuse with the sewage makes it impracticable to purify the sewage either upon land or by means of artificial processes, although in certain extreme cases special processes of preliminary treatment may be necessary."

UNIVERSITY AND EDUCATIONAL NEWS

By the will of the late Mrs. Jane A. Townsend, Yale University received $50,000 for the endowment of a professorship of history.

Maryville College at Knoxville, Tenn., has received $5,000 from Mrs. William Thaw, of Pittsburgh.

A fire on the fourth floor of McCoy Hall of the Johns Hopkins University, on the night of September 17, destroyed valuable manuscripts and archeological collections and damaged a large collection of books and pamphlets.

There were 137 students in attendance at the graduate school of agriculture, held this year at Cornell University, in addition to regular students of the university. In the summer session of the university there were 841 students.

According to a press cablegram, the chancellor of St. Petersburg University, Professor Ivan Ivanovic Borgmann and the vice-chancellor, Professor Fedor Alexandrovic Braun, have resigned from the institution. The faculty of the university has sent a collective declaration to M. Schwartz, the minister of education, stating that his recent repressive measures against professors and students endanger peace at the university, and declines to accept the responsibility for disorders that may occur.

In stating that in 1907 there were in Europe 125 universities, which were visited by 228,-732 students, Vice-Consul James L. A. Burrell, of Magdeburg, sends details. Of these the university of Berlin had the largest number of students, viz., 13,884; next came Paris with 12,985, Budapest with 6,551, and Vienna with 6,205. The list by country follows:

Country	No. of Universities	Students
Germany	21	49,000
France	16	32,000
Austria-Hungary	11	30,000
England	15	25,000
Italy	21	24,000
Russia	9	23,000
Spain	9	12,000
Switzerland	7	6,500
Belgium	4	5,000
Sweden	3	5,000
Roumania	2	5,000
Holland	5	4,000

The smaller countries—Greece, Norway, Portugal, Denmark, Bulgaria, and Servia—have each one university.

Dr. William Osler, regius professor of medicine at Oxford University, has been elected lord rector of Edinburgh University.

Dr. Harry A. Garfield will be installed as president of Williams College on October 7.

Mr. Robert Forsyth Scott, the author of works on mathematics, has been elected master of St. John's College in place of the late Rev. Dr. Charles Taylor.

The trustees of the University of North Carolina have made the following appointments: Professor Charles H. Herty to be

dean of the School of Applied Science; Associate Professor J. E. Latta, professor of electrical engineering; Professor A. H. Patterson, formerly of the University of Georgia, professor of physics; Associate Professor W. C. Coker to be professor of botany; Associate Professor Archibald Henderson to be professor of pure mathematics; instructors in mathematics, G. K. G. Henry and J. C. Hines, Jr.; instructor in physics, T. J. McManis. The university has just completed at the cost of $35,000, a new laboratory for the department of biology.

RECENT appointments at the University of Kansas are as follows: L. D. Havenhill, professor of pharmacy; assistant professors, G. W. Hartwell in mathematics, Burton McCullum in physics, H. C. Allen in chemistry, and A. H. Sluss in mechanical engineering; instructors Paul Wernicke, Mayer Gaba, C. A. Pierce in mathematics; F. U. G. Agrelius in botany; R. L. Moodie in zoology; Cecil Smith in physiology, and C. H. Wittington museum assistant in entomology.

THE following appointments have been made at Lehigh University: Instructors, R. G. Fogg, B.S., in civil engineering; H. E. Hendricks, B.S., in civil engineering; H. A. S. Howarth, Ph.B., in mechanical engineering; F. T. Leilich, E.E., in physics; Edgar T. Wherry B.S., and Chester G. Gilbert, Ph.B., in mineralogy; Assistants: Walter K. Van Haagen, B.S., in chemistry; Edwin E. Reinke, B.A., in biology.

DR. S. N. TAYLOR, of Pittsburg University, has been appointed professor of electrical engineering at the University of Cincinnati.

WASHBURN COLLEGE, Topeka, Kans., has established this year a department of botany and zoology with Dr. C. H. Edmondson, of the University of Iowa, in charge of zoology and Dr. Ira D. Cardiff, University of Utah, in charge of botany.

MR. A. B. FRIZELL has been appointed professor of mathematics at Midland College, Atchison, Kansas.

IN Manchester University, Mr. J. E. Petavel, D.Sc., F.R.S., lecturer in mechanics and in meteorology and demonstrator in physics, has been appointed professor of engineering; Mr. C. H. Lander, lecturer in engineering; Mr. T. G. B. Osborn, lecturer in economic botany; Mr. F. H. J. A. Lamb, M.D., now demonstrator in physiology, Cardiff University College, senior demonstrator in physiology; Mr. A. E. Woodall, junior demonstrator in physiology; Mr. T. W. Todd, senior demonstrator, and Mr. E. E. Hughes, and Mr. S. H. J. Kilroe, junior demonstrators in anatomy.

DR. HEINRICH BURKHARDT, professor of mathematics at Zurich, has been called to the Technical Institute at Munich.

DISCUSSION AND CORRESPONDENCE

THE TEACHING OF MATHEMATICS TO STUDENTS OF ENGINEERING

TO THE EDITOR OF SCIENCE: The observations of Professor George F. Swain, of The Massachusetts Institute of Technology, in the issue of August 28, on "The Teaching of Mathematics to Students of Engineering," are as valuable and suggestive as they are frank and progressive. They stand out clearly as the practical judgment of one in close touch with the needs of engineering. While these observations touch primarily the field of mathematics, and applied mathematics, and while we are compelled to let each specialty speak for itself; yet the same ideas, of using school training as a tool for practical use, and the necessity of developing the practical imagination, these ideas are quite as essential in other fields of natural science. As a teacher of chemistry, and one specially interested in the newer industrial and trades-school movement, I wish to emphasize the value of Professor Swain's remarks for chemistry in particular, and, presumably, for most of the other sciences in general. The contrast of view between the remarks of Professor Schlichter and Professor Swain is obviously that between the traditional teacher and the progressive engineer. The one looks at science from the standpoint of the teacher of theory; the other, from that of the user of school training. And in this difference, as clearly shown by Pro-

fessor Swain, is the new suggestion which is probably destined to be the basis of all industrial training. In a word, it is the pedagogical idea that the practical man learns by using. The question, "What is it good for?" has often been feared and avoided as the badge of cheap superficiality, on the one hand; and, on the other hand, as a serious menace to sound and honest research. But are we not now in a position where we may safely trust the well-trained teacher to use the actual need, and to employ the use of a science, as guides in teaching and learning that science? The immense advantage to be gained, for many students, by the combined assistance of the eye and the hand suggests that it may be best to start with actual tests and problems. This will place the student squarely face to face with facts and needs. This will also quicken the interest of the student; and it may prove to be the perpetual provider of a keen interest, that subtle psychological stimulus which spurs every good worker to success. Once given this start, and with the well-trained teacher, the student is naturally led to the helpful guidance of books and theory. Necessity is still the mother of progress; and we need not fear the sad augury implied in Professor Schlichter's remarks on teaching "dyeing and not chemistry." The student need not be "out of date," either at the start or later, if he is naturally led to the books and literature on his special field. Moreover, the student who starts with the practical, is always in touch with the actual needs of his craft—something which is often a sealed book to the theorist. It is not to be denied that this reversal of the application of theory to fact and need has its difficulties and dangers; so do all systems of education. But it looks like the solution of the technical and industrial education problem. It is to be hoped that this idea will not be allowed to slip from the attention of educators—the idea of using the fact, the problem, the need, the experiment—as the natural starting point for education, for teaching theory, and to catch the interest of the student. It bears three marks of genuineness, namely: It meets the practical needs; it catches the interest of the student; and it exemplifies the inductive method of learning through use.

CHARLES S. PALMER

23 PARK PLACE,
NEWTONVILLE, MASS.,
August 29, 1908

HUMMINGBIRD AND HORNET

EARLY in the summer of 1907 a dish of sweetened water was placed on the railing of the veranda of a cottage in North Acton, Mass. The next morning a female hummingbird was seen hovering over it. In a few days she became so accustomed to the presence of the family that she would feed from the vessel while a number of persons were sitting only a few feet away.

This year (1908) the cottage was first occupied on June 2. The next morning a pair of hummers were seen hovering over the railing where the sweetened water had been placed the year before. A saucer of water containing a few lumps of sugar was immediately provided for them.

They helped themselves frequently from this for several days, when the male disappeared. The female has continued her visits to the saucer many times each day up to the present time (August 24).

On July 22, while sitting within five feet of the vessel, I noticed, for the first time, a bald-faced hornet (*Vespa maculata*) inside the saucer. As I watched its motions, the hummingbird appeared, hovering over its accustomed feeding place. Instantly the hornet darted at it, and the hummer fled, closely pursued by the insect. The spectacle exactly resembled, on a small scale, the driving of a hawk or crow by a kingbird. In a minute or two the hornet was back exploring the contents of the saucer.

Presently the hummer returned, poised itself over the tempting dish, long enough to see that its enemy was on the ground, when it fled precipitately. She still (August 24) continues to come many times each day, only attempting to feed when the field is clear of hornets.

CHARLES W. MEAD
AMERICAN MUSEUM OF NATURAL HISTORY

QUOTATIONS

THE NEW BRITISH PATENTS ACT

THIS act, the Patents and Designs Act, became operative on August 28. Its principal clause runs as follows: "At any time, not less than one year after the passing of this act, any person may apply to the Comptroller for the revocation of the patent, on the ground that the patented article or process is manufactured or carried on exclusively or mainly outside of the United Kingdom." In future, foreign manufacturers, if they wish their patents to remain valid in Great Britain, will have to make the goods they sell within the United Kingdom. Otherwise their patents may be copied or infringed at will. Germany and the United States are particularly hit by the new enactment, and they are meeting the altered conditions by (1) building factories of their own in England; (2) acquiring premises already built for the purpose of carrying on their business; (3) arranging with British manufacturers to lay down plant and cooperate in the production of the special articles which are the subject of the patent. Already some thirty foreign firms—many of them conducting operations on a large scale—have begun, or are about to begin operations in this country, most of them choosing the north of England as the scene of their operations. It is said that as a rule the foreign manufacturer is providing a factory many times larger than is really necessary for the construction of his patented article, his explanation being that he can not run works in England on patents alone, and he intends therefore to manufacture in this country goods that have hitherto been imported ready-made. So far as can be seen at present the act must profit British labor. It is said in some quarters that these manufactures, at any rate the German ones, will be worked by foreign staffs, but this is not the case at present with Messrs. Meister, Lucius and Brüning (Limited), of Germany, a company with a capital of £11,000,000, which has just erected a new chemical factory at Ellesmere Port. Here all the workers employed are English, with the exception of a few German overseers. The working of the act will be watched with keen and anxious attention, for British manufacturers are beginning to realize that foreign competition is about to invade their own particular territory, and that there will be a fair but strenuous fight on British soil for British custom. That is not a prospect that can be viewed altogether without anxiety when the perfection of German organization is remembered. The German things to be manufactured in England will be mostly aniline dyes, pottery, plants for gas making, rifles, plated goods, electrical contrivances, furnaces, sanitary appliances; the American, typewriters, safety razors, phonograph records, shoes, telephones and wire roofing.—*Journal of the Society of Arts.*

SCIENTIFIC BOOKS

The Physiology of the Stomata. By FRANCIS ERNEST LLOYD. Pp. 1–142; f. 40, pl. 14. Carnegie Institution of Washington, Publication, No. 82.

The purpose of this study has been twofold: first, to determine to what extent the stomata are able to regulate transpiration; secondly, to ascertain the physiological cause of stomatal movement. The investigation was carried on almost exclusively with two desert plants, *Fouquieria splendens* and *Verbena ciliata.* Both of these plants were found to have leaves of the usual tropophytic character and without any of the obvious adaptive characters related to desert conditions. The rate of transpiration was determined by reading the volume of water absorbed from burettes to which cuttings of the plants were attached. By weighing any error due to the absorption of water by the tissues of the shoot or its loss by wilting was corrected. To determine the area of the stomatal openings at various times of day and so to correlate the movements of the guard-cells with the fluctuations of transpiration, portions of the epidermis were removed and fixed in absolute alcohol. It was found that this treatment had no appreciable effect upon the guard-cells and

consequently the exact area of the stomatal openings could be determined at any moment desired. The experiments revealed no correlation between the daily periodicity of transpiration and stomatal movement. On the other hand, it was first of all found that the rate of transpiration increased for a considerable time after the maximum stomatal opening in the early morning and that finally the rate may undergo sudden and wide changes without the accompaniment of a sufficient change in the dimensions of the stomata to account for them on the theory of stomatal regulation of transpiration. This latter result is in accord with the conclusions of Brown and Escombe who have found that the diffusion capacity of the stomata are quite generally greatly in excess of the actual maximum rate observed.

The experiments conducted under constant conditions demonstrated that transpiration is a physiological process, and not a physical one and that it is not to be looked upon as a necessary evil with which the plant has to contend. The rhythm of transpiration under constant conditions could not be correlated with stomatal movements and indeed it was later found in the case of wilting leaves that the beginning of the closure of the stomata occurs somewhat later than the initial wilting of the leaf and this movement appears rather as a result of the loss of water by the leaf as a whole than as a response in anticipation of wilting. The results of all the experiments indicate that the stomata are not adaptive structures in the active sense and if a regulation of transpiration exists it is effected by other means.

A valuable portion of the study is to be found in the second part of the work, dealing with the physiology of the guard-cells. By comparisons of the contents of the guard-cells taken at periods corresponding to the stomatal movements observed in connection with the work on transpiration, the writer found that these movements are correlated with marked changes in the nature of the cell contents. Thus it was found that starch begins to accumulate in the guard-cells in the afternoon, the maximum amount being observed in the night, while during the earlier hours of the morning the starch largely disappears, globules of oil, frequently one in each guard cell, taking its place. The movements and periods of stacis of the stomata were closely correlated with these fluctuations of the starch contents and it is inferred that the disappearance of the starch and the openings of the stomata are connected with the action of some unknown ferment. This conclusion necessitated the hypothesis that the metabolism of the guard-cells is radically different from that of the mesophyll cells and evidence was found to warrant this conclusion. The guard-cells were seen to accumulate starch at a time when it was disappearing from the ordinary chlorenchyma and on the other hand they were quite free of starch when photosynthesis was most active. This difference in function was further emphasized by experiments in which the leaves were exposed to the blue end of the spectrum, to darkness and to air devoid of carbon dioxide—under all of these conditions the plastids of the guard cells continued to accumulate starch though photosynthesis was impossible. It is maintained that the starch occurring in the guard cells is derived from the carbohydrates in the mesophyll and that the function of the chlorophyll in the guard-cells is in part, probably largely, secretory. It becomes necessary in accepting this hypothesis on the rôle of the guard-cells to assume that the ferment operative in the transformation of starch is of a radically different nature from other amylases since it is absent or inactive during the night and because of its marked activity during the earlier morning hours.

In conclusion the author finds no evidence in the behavior of the stomata studied to justify the conclusion that they in any way adapt these plants to the unfavorable conditions of the desert. He holds that the prevalence and magnitude of the devices that characterize xerophytes does not indicate that these plants have become fitted to their environment, but being fitted, they have survived. It must be conceded, however, that practically

identical xerophytic characters occur under a wide range of external conditions that are physiologically equivalent.

CARLTON C. CURTIS

The Principles of Direct-current Electrical Engineering. By JAMES R. BARR, A.M.I.E.E., Lecturer in Electrical Engineering, Heriot-Watt College, Edinburgh. New York, The Macmillan Company; London, Whittaker & Co. 1908. Pp. viii + 551; 294 illustrations.

There are several ways in which the general subject of electrical engineering may be divided for study or treatment in text-books. One very general scheme is first to take up the study of direct-current phenomena as applied to direct-current machinery, then to consider the study of alternating currents and alternating-current machinery, and finally to study the subject of transmission and distribution of power by both direct and alternating currents. A second method of division is to consider direct currents as a special case of periodic currents and to make the general division of the study of generators and receivers between induction apparatus and synchronous machines. Here again the subject of transmission and distribution is treated after a study of the machinery of both classes. A third classification consists of dividing the general subject into direct-current engineering and alternating-current engineering, treating under each head the generators, receivers, and systems of distribution utilizing direct currents or alternating currents as the case may be. For those who prefer the third classification the author has prepared a volume on the first division which should find a considerable application in colleges and technical schools.

The general method of treatment is not different from that used by other authors who prefer to consider direct-current engineering as separate from alternating-current phenomena. The first chapter is devoted to a review of the subject of units used, the relation of all practical units to the fundamental units being carefully stated. This is followed by chapters dealing with the laws of the electric circuit and the magnetic circuit, but before the application of these laws to the direct-current generator is taken up in detail a carefully written chapter on measuring instruments, in which the principle of operation and the sources of error of most of the instruments in common use are considered, is introduced, and this is followed by a brief study of the storage battery, electric lighting and cables. Three chapters are devoted to the direct-current generator, and in these three chapters the author has placed in a logical manner most of the information desired by those not interested directly in the details of designing. The subjects of motors and boosters are similarly treated and the book is completed by chapters on testing and electricity control, the final chapter setting forth the general principles involved in the design of the switchboard and of protective apparatus.

From the beginning the book deals primarily with the principles involved, the details of apparatus being introduced as illustrations of the manner in which the principles are applied rather than for the purpose of furnishing a catalogue of apparatus. To further aid the student in making application of general principles to calculations, carefully prepared problems with their complete solutions are introduced at intervals throughout the text, and similar problems for solution by the students themselves are stated in an appendix. The problems as given are practical and the illustrations of machinery and instruments are taken from modern practise. The use of two colors in the diagrams of armature windings and other connections should aid the student greatly in his study of the subject. The index of the book is complete enough to make it a ready work of reference.

GEO. C. SHAAD

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
June 26, 1908

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Experimental Zoology, Vol. V., No. 4 (June, 1908), contains the following papers:

The Chromosomes in Diabrotica vittata, Diabrotica soror and Diabrotica 12-punctata: N. M. Stevens.

Diabrotica vittata has an unpaired heterochromosome which passes undivided to one pole of the first spermatocyte spindle and divides in the second maturation division. *Diabrotica soror* and *Diabrotica 12-punctata* have, in addition to the unpaired heterochromosome, in about fifty per cent. of the male individuals collected, one, two, three or four small "supernumerary" heterochromosomes, the number being constant for the individual. The supernumeraries divide sometimes in the first, sometimes in the second spermatocyte mitosis.

The Experimental Control of Asymmetry at Different Stages in the Development of the Lobster: Victor E. Emmel.

In the adult lobster asymmetry of the chelæ is very stable and not subject to reversal, but in the first four larval stages it was found that right or left asymmetry can be produced at the will of the experimenter; consequently it appears that the possibility for experimental control of asymmetry is correlated in some way with the degree of differentiation or development of the organism. These facts indicate that the factors controlling asymmetry become operative after the organism leaves the egg, and that "right- or left-handedness" is not necessarily a question of "inheritance" or even of "alterations in germinal organization."

The Physiological Basis of Restitution of Lost Parts: C. M. Child.

The paper includes a discussion of Holmes's hypothesis of form-regulation and a statement of the writer's position regarding the physiological basis of the process of restitution, which is that a lost part can be replaced only when some other remaining part is physiologically sufficiently similar to it to perform its chief functions qualitatively if not quantitatively, after its removal.

The Process of Heredity as exhibited by the Development of Fundulus Hybrids: H. H. Newman.

Heredity is conceived of as essentially a resemblance in developmental process between offspring and parents and is studied experimentally as such.

In hybrids between these two species of fish the earliest disturbances of the normal developmental process produced by the introduction of foreign spermatozoa are noted, the origin and rhythmic flux of characters are studied, and attempts are made to get some light on the ultimate physiology of the process. Accompanying the paper are pictorial tables showing the comparative developmental processes of the two pure breeds and the reciprocal crosses.

Variation, Heredity and Evolution in Protozoa. I. The Fate of New Structural Characters in Paramecium, with Special Reference to the Question of the Inheritance of Acquired Characters in Protozoa: H. S. Jennings.

The author followed the fate at reproduction of many new or "acquired" structural characters, some produced experimentally, some found in nature. These were not inherited. Sometimes such a character is handed on bodily to a single individual of each generation; one was thus followed for twenty-two generations. But there is no tendency for them to multiply and produce a race bearing them. Such a tendency shows itself only in the case of the very rare characteristics arising from something permanently modifying the process of fission. "The inheritance of acquired characters" takes place no more readily nor generally in protozoa than in higher organisms.

LITHIUM IN RADIOACTIVE MINERALS[1]

The question as to whether lithium is or is not a widely occurring element, and whether it is found associated with any other element, more particularly with copper than with the alkalies or the alkaline earths, arises from the assumed transmutation of copper contained in solutions, into lithium, neon and possibly other substances.

[1] Abstract of a paper by Professor W. N. Hartley at the Dublin meeting of the British Association.

It has been stated by Sir William Ramsay:[3] "As sodium and potassium are much more widely distributed than lithium, it is more likely that they are the chief products from copper, and that some modifying circumstance has determined the formation of a trace of lithium. . . . Lithium was mentioned because it is an unlikely constituent of dust, glass, copper, etc., which were tested specially to prove its absence."

There are two statements here which, according to my experience, appear to require modification. That potassium and sodium are more abundantly distributed than lithium is true, but that these are more widely distributed is not strictly correct; nor can it be accepted as unquestionable that lithium is an unlikely constituent of dust, glass, copper, etc. Evidence to the contrary is based upon facts divided into three categories—firstly, those derived from the qualitative spectroscopic analysis of common ores and minerals usually associated with the alkali metals; secondly, analysis of the crude salts of the alkalies, such as the Stassfurth minerals and nitrates from Chili and Bengal, show that they contain lithium and rubidium, with not unfrequently cæsium. Facts belonging to the third category are derived from experimental evidence, which is both quantitative and spectrographic, the source of the spectra being the oxyhydrogen flame. When half a gram of material yields a photograph of the spectrum of lithium on which the four chief lines are visible—namely, $\lambda\lambda$ 6708, 4603.07, 4132.93 and 3232.82—there cannot be less than 0.0089 gram of lithium present. When only the lines 6708.0 and 4603.07 are visible, the quantity is not less or more than 0.0041 gram.

When only the red line is photographed the quantity is not more than 0.002 gram, and with half this quantity the line ceases to be photographed. It follows, therefore, that from the evidence afforded by the number of plates on which this line appears there could scarcely be less lithium in the 0.5 gram of material analyzed than 0.2 per cent.

Further results have been obtained with several other metallic compounds, but the sensitiveness of the flame reaction varies extraordinarily with the spectra of different elements.

Mr. Ramage and I found in 170 common ores and minerals potassium and sodium, and with these common elements rubidium and lithium were very generally associated. Thus, of sixty-two iron ores, rubidium was found in sixty-one. In sixteen red hæmatites, massive minerals of the purest type, rubidium was contained in four. Where potassium and rubidium occurred lithium was invariably found. It was found in limestones, in dust, in the Bessemer flame, in ordinary pipeclay, tobacco pipes, and a great variety of siliceous minerals, such as the Dublin granites; in Donegal kyanite, which contains 98 per cent. of aluminium silicate; and in asbestos. It was found in dust which fell from the clouds, in volcanic dust, in soot, in flue-dust from chemical works, and in that from copper smelting and refining works. This last material contained lithium, sodium, potassium, rubidium and cœsium, copper, silver, calcium, strontium, aluminium, gallium, indium, thallium, iron, nickel, cobalt, manganese, chromium, lead, zinc, cadmium and tin. Upon such evidence as this it is impossible to corroborate the statement that potassium is a more widely distributed element than lithium, or that lithium is an unlikely constituent of dust, glass, copper, etc.

[3] *Nature*, March 5, 1908, p. 412.

SPECIAL ARTICLES

ON THE ORBITOSPHENOID IN SOME FISHES

I WISH to call attention to the following paragraph recently published by Dr. L. S. Berg[1] in reference to an orbitosphenoid alleged by different authors from time to time to exist in various fishes.

> Das Orbitosphenoid fehlt bei allen untersuchten Formen. Prof. Starks, der diesen Knochen bei den Fam. Berycidae und Monocentridae fand, sagt in seiner interessanten Abhandlung folgendes:[2]

[1] "Die Cataphracti des Baikal-See," p. 26. Wissensch. Baikal-See Exped., Lief III., 1907.

[2] *Proc. U. S. Nat. Mus.*, XXVII., 1904, p. 601. "It is remarkable to find this archaic character

among the spiny rayed fishes, though it is well in keeping with the pneumatic duct to the œsophagus, which some of the Berycoid fishes are said to have. The presence of orbitosphenoids is common among the lower forms from the bony Ganoids up to and including the Salmoids. So far as the author can ascertain, they hitherto have not been found in forms more specialized than the last. They have been searched for in vain in the following: *Aulopus, Synodus, Esox, Fundulus, Aphredoderus* and nearly all of the families of the Hemibranchs, Synentognaths and the Percesoces." Den gegenüber kann man einwenden, (1) dass das Orbitosphenoid nicht allen niedrigeren Teleostei eigenthümlich ist. So fehlt es unter den Malacopterygii bei *Osteoglossum, Gonorhynchus, Chanos,*[3] *Cromeria,*[4] unter den *Cobitidini* bei *Cobitis, Misgurnus, Acanthophthalmus,*[5] und (2) dass ein Orbitosphenoid bei mehreren Formen, die im System höher als die Salmoniden stehen, bekannt ist. So besitzt nach Vrolik[6] *Aulopus filamentosus* (*Scopelidae*) ein "sehr ausgedehntes Orbitosph" (p. 270, Taf. XX., Fig. 30). Ferner ist dieser Knochen bei *Galaxias*[7] vorhanden, ebenso unter den Acanthopterygii: bei *Micropterus salmoides,*[8] bei *Pomacanthus paru,*[9] bei *Grammicolepis,*[10] bei *Regalecus.*[11] Wir finden folglich das Orbitosphenoid unter den Acanthopterygii bei den verschiedensten Familien, von so niedrig organisierten wie die *Berycidae* angefangen bis zu so hohen wie die *Trachypteridae.* Weitere Forschungen werden wahrscheinlich zeigen, dass dieser Knochen noch mehr verbrietet ist, als man früher angenommen hat.

Referring to part 1 of Dr. Berg's conclusions I wish to point out that in my above quotation I said that an orbitosphenoid was *common* in fishes below the Salmonidæ, not that it was always present as Dr. Berg appears to have inferred.

In reference to part 2 of his conclusions I have carefully examined a number of specimens of both *Micropterus* and *Pomacanthus* and several of their close relatives, and find no orbitosphenoid, nor is there any mention of this element in any other report on these forms. The common yellow perch is a close relative of *Micropterus,* and since the time Cuvier first took it as an anatomical type of spiny-rayed fishes its skeleton has been described a great many times, but without mention of an orbitosphenoid. Dr. Boulenger, in the first volume of the second edition of the "Catalogue of Fishes in the British Museum" has worked out the osteology of a great number of the basses, perches and sun-fishes, but without finding an orbitosphenoid.

Either the internal descending wing from the frontal, or the anterior part of the alisphenoid, has been mistaken for this element in *Micropterus* and *Pomacanthus.* There is often a slight mark across the alisphenoid which may have been interpreted as a suture dividing the bone into two parts. This appears to have been the case in *Grammicolepis,* to judge from the picture, though I have had no opportunity for examining the skeleton. As to *Aulopus,* my specimen (*A. japonicus*) certainly has no orbitosphenoid, though the picture of *A. filamentosus* published by Vrolik shows a well-developed one. It seems probable that somewhere there has been a misidentification of material. Dr. Berg has misinterpreted Dr. Swinnerton's statement in regard to *Galaxias.* Swinnerton referring to this genus, says, "owing to the absence of an orbitosphenoid [etc.]." On the preceding page, however, he gives a figure of the cranium of *Galaxias* in the anterior part of the orbital cavity of which is a portion marked "os." To "os" I find no reference in the text, though it may as well refer to a cartilaginous or membranous orbital septum as to an ossified orbitosphenoid.

[3] Ridewood, *Proc. Zool. Soc. Lond.*, 1904, p. 59; *op. cit.*, 1905, p. 485, fig. 140–1, p. 489.

[4] Swinnerton, *Zool. Jahr. Abt. Anat.*, XVIII., 1903, p. 63, fig. F.

[5] Sagemehl, *Morph. Jahr.*, XVII., 1891, p. 579.

[6] *Neid. Arch. für Zool.*, I., 1873.

[7] *Haplomi:* Swinnerton, *Zool. Jahr. Abt. Anat.*, XVIII., 1903, p. 63, fig. G.

[8] Centrarchidæ, Shufeldt, U. S. Fish Comm. Rept. 1883, XI., 1885, p. 804.

[9] Chætodontidæ, Shufeldt, *Jour. Morph.*, II., 1889, p. 290, fig. 10.

[10] Zeidæ, Shufeldt, *Jour. Morph.*, II., 1889, p. 280.

[11] Trachypteridæ, T. Parker, *Trans. Zool. Soc. Lond.*, XII., 1886, p. 12, T. IV., fig. 7, 11.

Regalecus has a large orbitosphenoid, and Mr. Tate Regan has recently shown[12] that *Lampris* and *Velifer* also have one. I believe these and the Berycoid fishes to be the only spiny-rayed fishes in which the orbitosphenoid has been proved to exist.

Dr. Berg has apparently not appreciated the true significance of the presence of an orbitosphenoid in *Regalecus* when he remarks towards the end of his paragraph that this element has been found from so low a group as the Berycoids to so high a one as the Trachypteridæ. Instead of indicating that an orbitosphenoid may be looked for anywhere among the Acanthopterygii it rather indicates the primitive character of *Regalecus*. Mr. Regan (*op. cit.*) has, in fact, recently placed it in close relationship with the Berycoid fishes, but whether or not *Regalecus* (with its relatives forming the group Tænіosomi) originated from the Berycoid fishes, it is at least as primitive as they are, and belongs in the system not far from them. If it is true that *Grammicolepis* has an orbitosphenoid it would indicate its position also to be not far from the beginning of the series of spiny-rayed fishes.

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AN EXPLANATION OF THE CAUSE OF THE EASTWARD CIRCULATION OF OUR ATMOSPHERE

IN SCIENCE for December 20, 1907, I have shown that the principle of the conservation of energy demands that *temperature must be taken as a measure of the intensity of ether vibration;* this mandatory condition at once gives us the information that only the Newtonian law of radiation can be true, and this claim is upheld by my interpretation of existing observations (as explained in the closing paragraph of that paper). I then demonstrate that the absolute temperature of space at the earth's distance from the sun is probably less than two degrees centigrade.

As known gases become either liquid or solid when the temperature is reduced to within a few degrees of the absolute zero, a planet can have no atmosphere unless its surface-temperature is above the critical temperature of the gas which forms the atmosphere.

From the differences between the polar and equatorial temperatures near the earth's surface, and from the decrease in temperature with increasing height above the surface, it is known that the atmospheric layers near the surface of the earth act as a trap to retain the heat until the temperature reaches a limit which varies with varying atmospheric conditions; beyond this limit the loss of heat through radiation into space is just equal to the heat received, so that no farther increase in temperature takes place.

As the direct rays of the sun can strike only one half of the earth's surface at a given instant, while the equivalent heat is later on radiated from the whole surface of the earth, it is plain that the mean *solar* component of earth-radiation can not at its maximum exceed one half of the sun's radiant effect at the earth's distance from the sun, or 0°.75, if 1°.5 is adopted as the temperature of space; practically, therefore, the whole terrestrial radiation into space is due to inherent earth-heat.

Let us, provisionally, take it for granted that on the average the atmospheric layers near and in contact with the earth's surface have, by reason of the trapped heat, a temperature 100° higher than would be the case if no heat were stored in these lower layers, we then readily arrive at the results given in the following table:

Distance above Earth's Surface (in Miles)	Terrestrial Radiation		Temperature of Direct Solar Rays	Gravity
	Earth's Component	Sun's Component		
0	200°.	0°.7	0°.7	1.00
10	199.	0.7	1.+	0.99
100	190.	0.7	1.+	0.95
1,000	128.	0.6	1.+	0.64
10,000	16.	0.1	1.5	0.08
100,000	0.1	0.0	1.5	0.04

From an inspection of the above table we learn that during the first few hundred miles the decrease in temperature, due to radiation, is only one degree for each additional ten

[12] *Proc. Zool. Soc. Lond.*, 1907, pp. 634–643.

miles of altitude, so that in the higher available regions of the atmosphere (where the decrease in the stored heat, for accessible increasing heights, is probably insensible) observational data should reveal a practically constant temperature for all superior distances that can be reached by the known means at our disposal. *The experimental results recently obtained by means of kites and balloons confirm in a striking manner the run of the data given in the above table,* in which the *earth,* not the sun, is taken as the controlling influence so far as temperature conditions are concerned.

The ever-varying unstable conditions in the lower strata of high temperature cause more or less continuous ruptures of these strata, each vent containing an uprush of the heated air fed by a horizontal inrush on all sides. In the equatorial regions the inrushing air has a more or less uniform temperature, and the direction of motion is nearly straight towards the axis of the uprush, so that great cyclonic motions are not to be expected as a regularly recurring phenomenon in these regions. In the middle latitudes, however, the conditions are always such that cyclonic movements of the lower air are almost inevitable.

Owing to the decrease in the diurnal surface-velocity of the earth with decreasing polar distance, an uprush of air in a middle latitude will, in general, be supplied as follows: on the equatorial side, by warm air rushing polewards, not directly towards the axis of the uprush, but always towards a region on the east side of this axis; on the pole side, by cold air moving equatorwards towards a region on the west side of the axis. Owing to this arrangement of the moving air, equilibrium can not at once be restored, and a great cyclonic motion of increasing intensity results, to be overcome later on by the destruction of the vertical motion through the now increasing want of sufficient air-pressure from below.

The cyclonic motion of the atmosphere, brought into action through the axial rotation of the earth, has long been known, but so far as I am aware no satisfactory answer has ever been given to the question—*Why does the atmosphere, taken as a whole, have a greater angular velocity of rotation (diurnal) than the earth itself?* I offer the following explanation:

An inspection of the above table shows that the uprushing expanding air may rise many hundred miles and still have a temperature far above the critical point.[1] *As the air-mass rises (and loses its moisture through condensation) its diurnal angular velocity diminishes, so that by the time this same (now dry) air again reaches the lower layers of the atmosphere, to cause an increase of pressure, the region of the uprush will be far to the east of the place where the pressure has increased. To restore the equilibrium the piled-up mass of air now flows back into the region of low pressure farther to the east and thereby causes an eastward motion of the atmosphere with reference to the earth's surface.*

As each "low" is forced to move eastward by its necessarily following "high," the general eastward circulation of our atmosphere is explained. It is evident that the observed equatorial acceleration of the sun's atmosphere, and of planetary atmospheres in general, can be explained in a similar manner.

In an atmosphere quiescent throughout, the different gases constituting the envelope would be arranged in concentric layers, the lightest gas being at the top; through the vertical circulation, however, a mechanical mixture of these gases must take place, and other phenomena must also result.

J. M. SCHAEBERLE

ANN ARBOR,
March 24, 1908

[1] It is proper to state here that during the progress of my investigations it was found necessary to reject the kinetic theory of gases and to substitute in its place a simpler and more rational theory, which is so general in its application that even gravitation is satisfactorily accounted for. According to this theory the force which causes an uprush, or which causes radiation in general, has the same source and the same properties as the force which causes gravitation and other physical phenomena of nature.